DISCUSSION PAPER

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LABOR ENFORCEMENT AND FORMAL EMPLOYMENT: THE EFFECTS OF COMMUNICATION AND PUNISHMENT

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CONTENTS

ABSTRACT

We evaluate the effects of a large-scale enforcement intervention on formal labor flows. The initiative combined a communication component (an official letter/e-mail sent to registered employers) and a punishment component (face-to-face inspections). Using two identification strategies (regression discontinuity design – RDD and difference-in-differences – DiD) we isolate the effects of each component keeping the other constant. Results show that both components increased the formalization of previous informal workers but did not change regular, formal labor demand. Effects are observed only for the short run, indicating that employers reacted to each component in the aftermath of the intervention and then moved back to their usual compliance behavior.

Keywords: enforcement intervention; labor inspection; formal employment.

SINOPSE

Este estudo avalia os efeitos de um programa de larga escala de inspeção do trabalho sobre fluxos de emprego formal. O programa combinou um componente de comunicação (uma carta/e-mail oficial enviado a empregadores formais) e um componente de punição (inspeções face-a-face a empresas formais). Usando duas estratégias de identificação (design de descontinuidade de regressão (regression discontinuity design – RDD) e diferença nas diferenças (difference-in-differences – DiD), isolamos os efeitos de cada componente mantendo o outro constante. Os resultados mostram que ambos os componentes aumentaram a formalização de trabalhadores previamente informais, mas não alteraram a demanda por trabalho formal. Os efeitos são observados apenas no curto prazo, indicando que os empregadores reagiram a cada componente durante a intervenção e depois retornaram ao seu comportamento usual de cumprimento da legislação.

Palavras-chave: intervenção de cumprimento; inspeção do trabalho; emprego formal.

1 INTRODUCTION

High rates of labor informality are a conspicuous and persistent phenomenon in developing countries. According to ILO (2018), the share of informal employment in developing and emerging countries was around 60% of total employment and 50% when only employees are considered. For Latin America, these figures were 54% and 38%, and for Brazil, where our data come from, 46% and 34%, respectively.

There is wide consensus that labor informality has widespread social and economic implications. First, informal workers are not protected by labor rights, have lower entitlement to social security benefits, and are likely to experience scarring effects in the labor market from having worked as informal workers (Cruces, Ham and Viollaz, 2012; Pritadrajati, Kusuma and Saxena, 2021). Second, informality is harmful for productivity, for instance because it distorts firms' decisions to invest and grow larger (Ulyssea, 2018). Third, tax evasion leads to lower government revenues hindering the provision of public goods. Fourth, and counterbalancing its negative implications, it is accepted that informality provides flexibility in the labor market, which helps modulating the negative effects of unemployment, especially after the occurrence of adverse shocks (e.g., Perry et al., 2007; Ponczek and Ulyssea, 2021).

There is much less consensus on the relative effectiveness of the various policy instruments to reduce labor informality. This is partly explained by the large range of policies and programs implemented by governments of low and middle income countries to increase formality. This paper is connected to one of the most widely used instruments, namely: labor inspections and enforcement interventions.[1]

Specifically, we evaluate the effects of a large-scale initiative launched by the Brazilian federal government in 2014 to combat informality of salaried workers in the country. The initiative combined two different components. One consisted of sending e-mail messages or letters to tax-registered establishments containing two elements: i) a moral suasion element emphasizing the social importance of registering workers and the negative effects of informal hiring on fair competition; and ii) a deterrence element

1. Recently, Jessen and Kluve (2021) have categorized the multitude of policy instruments and initiatives into five types of interventions. The first is the provision of information to employers and entrepreneurs on the registration process and the benefits of being registered. The second is simplification/registration interventions which aim at simplifying formal entry. The third, which is typically coupled with the previous two, is the provision of financial incentives to reduce the costs of registration. The fourth is the concession of tax incentives such as reduction in labor taxes to lessen the tax burden on firms. The fifth is labor inspections and enforcement interventions that aim at increasing compliance with firm or worker registration.

informing employers on the modernization of the labor enforcement system and the potential penalties associated with non-compliance. No threat of future inspections was included in the letter/e-mail. We name this component the communication component. The second component was the intensification of face-to-face inspections in tax-registered establishments specifically targeted to enforce the registration of workers. We refer to this component as the punishment component.

In conducting the evaluation of the program, we try to answer two questions based on the following thought experiment: i) for a given level of inspections, what happens to formalization of employed workers after an enforcement intervention based on the communication component?; and ii) given the receipt of the communication component, what happens to formalization of employed workers if the intervention includes an increase in the punishment component? Exploiting the differential increases in inspections, we also attempt to measure potential heterogeneous impacts of distinct levels of increase in the punishment component.

Most of the literature in labor and development economics focuses on the informality and unemployment effects of face-to-face inspections alone (Ronconi, 2010; Almeida and Carneiro, 2009; 2012; Bhorat, Kanbur and Mayet, 2012; Almeida, Carneiro and Narita, 2015; Meghir, Narita and Robin, 2015; Viollaz, 2017; Pignatti, 2018; Abras et al., 2018; Ulyssea, 2010; 2018; La Parra and Bujanda, 2020; Haanwinckel and Soares, 2021; Ponczek and Ulyssea, 2021; Dix-Carneiro et al., 2021). There are few studies that investigate the effects of interventions that included the delivery of communication materials to firms or entrepreneurs. All these studies were targeted to fully informal firms or self-employed individuals, so their results are restricted to what the literature calls the extensive margin of informality. Andrade, Bruhn and McKensie (2013) conducted an experiment with different arms in which two of them involved an in-person delivery of a brochure to informal firms in one municipality in Brazil. The brochure included information on the advantages of being formal (e.g. availability of lines of credit and possibility of participating in public procurement) as well as the cost of informality (e.g., the risk of seizure of goods and application of fines). There were two other arms in the experiment based on inspections from the local authorities. The study finds no effect from the arms that delivered the brochure and positive effects from the arms based on inspections. Bosch, Fernandes and Villa (2015) measure the effect of a large-scale intervention in Brazil that consisted of sending an official booklet by postal mail to self-employed workers reminding them on the obligation to contribute to social security. The authors find an increase in compliance that took place in the month after the delivery of the booklet, an effect that disappears in the following months. Giorgi, Ploenzke and

Rahman (2018) study the effects of in-person delivery of an official letter to firms in Bangladesh threatening business owners with inspections and due penalties if they did not register their firms within a given time frame. The letter also informed business owners on the importance of registration for having access to benefits such as getting loans in the bank system and obtaining ownership of land and various types of licences. The study finds that the intervention increased the rate of registration of firms but does not detect spillovers effects on non-treated firms in the same locations of the treated ones.

In contrast to the field of labor economics, there is a large literature on the economics of taxation that deals with the effects of the communication component on the tax compliance behavior of individuals and firms (e.g., Slemrod, Blumenthal and Christian, 2001; Hasseldine et al., 2007; Kleven et al., 2011; Ariel, 2012; Ortega and Sanguinetti, 2013; Gangl et al., 2014; Pomeranz, 2015; DeBacker et al., 2018; Alm et al., 2019; Bergolo et al., 2023). Looking at changes in evasion of different types of taxes, this literature carefully investigates the effects of sending letters from the tax authority to agents. In general, distinct letters are used with some containing deterrence elements such threats of future auditing and some containing tax morale elements that typically appeal to non-pecuniary aspects of tax compliance. Results from this literature are mixed with some studies finding reductions in tax evasion and some zero or even negative impacts from the interventions.[2]

Our main contribution is to disentangle the effects of the communication and the punishment components on formal admissions and separations of workers. To the best of our knowledge, this is the first study that investigates the effects of each component (keeping the other constant) from a large-scale enforcement intervention that reached registered employers irrespective of size in a country. We believe this is important on a policy perspective since it adds evidence on how employers behave when faced with two different enforcement instruments. It is also directly relevant for governments as the costs of delivering the two components are very different. Providing evidence on the two effects also contributes to the literature on the impacts of enforcement interventions, including the tax compliance and the labor economics literature.[3]

The initiative we analyse took place in a set of 527 municipalities – around 9% of the 5,570 municipalities in the country – with less than 100,000 inhabitants. All tax

2. See Slemrod (2007; 2019) and Alm (2019) for thorough surveys of this literature.

3. The paper also connects to the literature on labor market institutions (e.g., Besley and Burgess, 2004; Botero et al., 2004; Boeri and Jimeno, 2005; Autor, Kerr and Kugler, 2007).

registered establishments in the chosen municipalities were sent the e-mail/letter of the communication component. In- crease in face-to-face inspections came after the mailing of the missives in just part of the municipalities treated by the communication component. The choice of the treated municipalities was also based on the rate of informality of salaried workers across the eligible municipalities. However, despite the use of objective criteria, other factors like political influences and the inspection capacity of the various offices in charge of conducting labor inspections throughout the country may also have played a role in defining treatment by the two components.[4] Thus, treatment by the communication or the punishment components may be endogenous to factors that drive the formalization of workers across the country.

We use two distinct strategies to identify the effects of interest. One is regression discontinuity design (RDD) which exploits the program's eligibility threshold of 100,000 inhabitants at the municipality level. In principle, potential effects are driven by the two components of the program at the cutoff point. However, our results show that the inspection rate did not change at the cutoff, so we argue that the measured impacts are based solely on the communication component (for a given level of inspection rates). The second method we apply is difference-in-differences (DiD) through which we compare treated municipalities (by the communication component) that experienced heterogeneous increases in the intensity of the punishment component. Specifically, using the distribution of changes in inspection rates across treated municipalities during the implementation of the program, we compare municipalities that were above some percentiles of that distribution with those of a control group that did not undergo an increase in inspections. Our outcome variables come from hiring and dismissals information provided by registered establishments to the government. Employers in Brazil have 30 days to inform the government when they hire or fire a worker, and after such a period they have to pay a fine. We use two sets of outcome variables based on the number of admissions and dismissals that took place either before or after the 30 days period. Though the provision of informa tion after the due date may reflect delays due to haphazard reasons, it may also capture changes in employers' behavior regarding the formalization of previously hired or dismissed workers. In fact, the labor inspection authority uses these numbers as indicators of their enforcement performance. Infor- mation on hires and dismissals provided before the due date is intended to capture changes in regular, formal labor demand over time. These outcome variables were aggregated

4. Brazil is a large and heterogeneous country and inspection capacity varies across the regions and states of territory. Cardoso and Lage (2005) and Almeida and Carneiro (2012) provide detailed descriptions of how labor inspections are organized in Brazil.

at the municipal level since the data on the number of inspections were only made available to us at this level of aggregation. The advantage of using aggregate data as opposed to firm level data is that our estimates include potential effects on non-treated employers in treated areas, i.e. we may also be gauging the effects of the intervention on the extensive margin of informality.[5]

The results from the RDD estimation show a substantial increase in the formalization of hires and dismissals of previously employed informal workers but do not reveal any changes in new hires and dismissals. As aforementioned, since there was no change in inspection rates at the program's population cutoff, we attribute the observed increase in formalization as the effect of the communication component alone. Potentially, the communication component could change employers' behavior with respect to adjustments in their regular, formal labor demand. But only formerly informal workers were formalized, so one possible explanation is that the communication component sparked fears that future inspections would take place and detect previous informal hires and dismissals but not new ones.

Similarly, DiD results show that hires and dismissals of formerly employed informal workers increased but they do not show changes in new hires and separations. This indicates that even when face-to-face inspections are part of the intervention, employers only changed their decisions regarding the stock of previous informal workers. Point estimates are higher for larger changes in inspection rates but they are not different from each other on statistical grounds. This reveals that the effects of increments in the intensity of the punishment component were not heterogeneous.

The effects of the communication or the punishment component are ob- served for the period of program's implementation but they vanish in the following periods. This suggests that employers reacted to the components of the program formalizing the stock of previously employed informal work- ers in the aftermath of the intervention and then moved back to their usual compliance behavior. This behavior is compatible with what the literature refers to as "action and backsliding" (Bosch, Fernandes and Villa, 2015).

Apart from this introduction, the paper contains another five sections. In the second section, we describe the intervention and explain in more detail the communication and

5. As our data are for tax-registred establishments, we mainly capture the effects at the intensive margin of informality, i.e. the impact for firms that do not necessarily register all their workers. But it is possible that through communication between formal and informal employers and the increase in inspections in the treated localities, fully informal entrepreneurs (i.e., the extensive margin) decided to formalize their businesses. See Ulyssea (2018) for an aggregate model where labor formalization is driven by the combination of the intensive and extensive margins of informality.

the punishment components. The third section describe the data and the fourth section spells out the two identification strategies we employ. Results from the two methods are presented and discussed in section five. Conclusions are presented in the last section.

2 THE INTERVENTION

Labor inspections in Brazil, as elsewhere, have the mandate to check compliance with the labor code. In Brazil, the labor code is extremely detailed, so labor inspection efforts pulverizes across several dimensions such as checking unregistered labor relations, misconducts on paid vacations, payment for extra hours, minimum wage provisions, lack of contributions for workers mandatory saving accounts, and compliance with safety conditions. Typically, labor inspections take place either following anonymous requests or through planned actions by the inspection authority. In both cases, the bulk of inspections is not focused on labor informality but on the aforementioned items (Cardoso and Lage, 2005; Almeida and Carneiro, 2012).

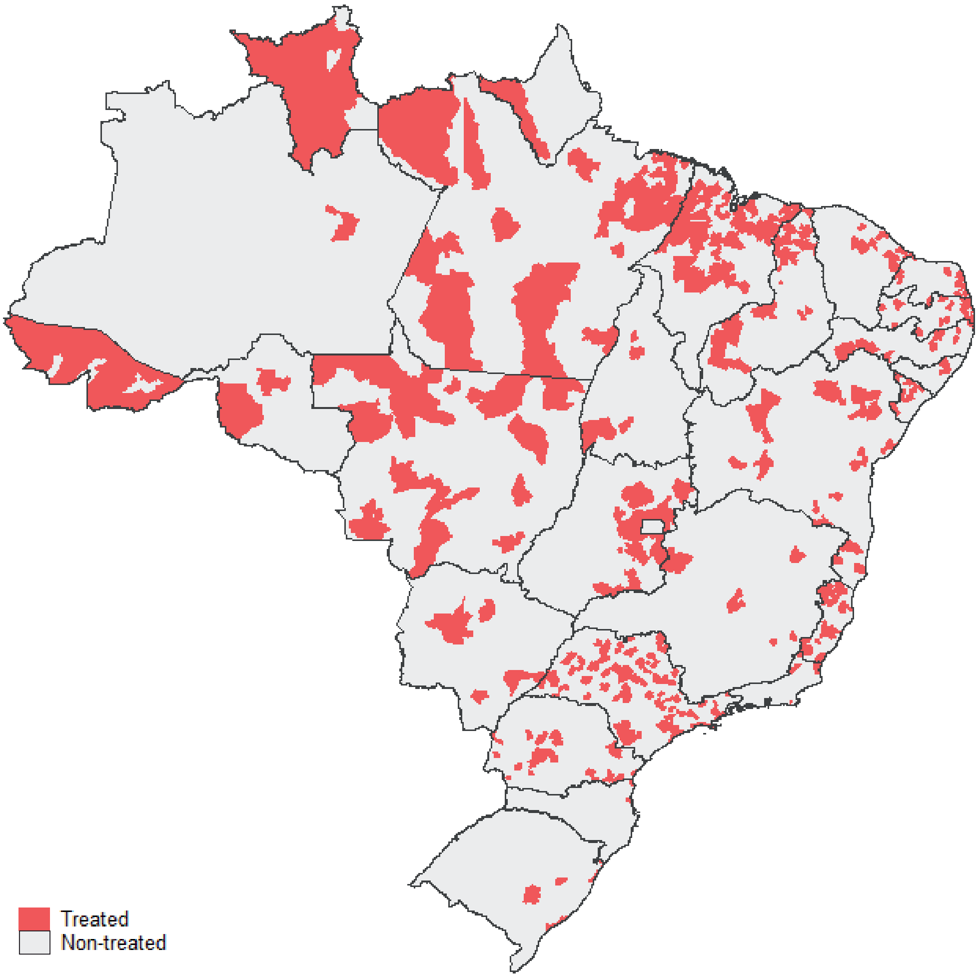
Within this context, the Brazilian government launched the National Plan to Combat Informality of Employees (Plano Nacional de Combate à Informalidade dos Trabalhadores Empregados – Plancite), the first large scale program specifically targeted to tackle the practice of firms to hire workers without a formal labor contract.[6] The intervention, launched in 2014, encompassed changes in multiple components including modernization of information systems, use of fiscal intelligence, specific training for inspectors on the new policy, dissemination of information to employers on the social importance of formal hiring and the costs of informality, and face-to-face inspections focused on detecting unregistered workers in tax-registered firms.[7] We study the effects of one of the first Plancite's initiatives which took place in the last quarter of 2014 and the first quarter of 2015. Eligibility was restricted to employers located in municipalities with less than 100,000 inhabitants according to the 2010 demographic Census. In total, 527 municipalities – around 9% of the 5,570 municipalities in the country in 2014 – were chosen to receive the program. As can be seen in figure 1, treated municipalities were spread over the entire territory of the country.[8]

6. In Brazil, informal employees are those whose employers neither sign their carteira de trabalho – a booklet that records all labor contracts of the worker in the formal sector – nor register them in the government's official systems.

7. Pires et al. (2017) discusses how the Plancite's proposed framework intended to innovate the policy to combat employee informality in the country.

8. It should be noted that the municipalities in the Midwest and specially in the North regions of Brazil tend to have large areas.

FIGURE 1
Location of treated municipalities in Brazil



Source: Secretaria de Inspeção do Trabalho (SIT).
Authors' elaboration.

The initiative comprised two of the previously mentioned Plancite's components. The first consisted of sending a letter or e-mail message to all tax-registered establishments (or their external accountants) located in the chosen set of municipalities.[9] The contents of the missives contained two elements commonly found in the literature on tax compliance (e.g. Slemrod, 2019; Alm, 2019). One element is based on moral suasion which emphasizes the social importance of registering workers – for instance, by providing them access to social security benefits – and the harm that hiring workers informally engenders on fair competition. The second is a deterrence element that

9. The inspection authority has a list of all addresses and e-mails of tax-registered establishments in the country. As many establishments only provide the contact of their external accountants, the letters or e-mails messages were sent to them in this case.

informed employers on the modernization of the enforcement system – for instance, use of fiscal intelligence and crossing of information amongst government agencies – and the potential penalties associated with non-compliance, including the possibility of losing access to public programs. The letter/e-mail did not mention that inspections could follow, so there is no explicit threat that employers could be inspected.[10] We name this component of the intervention the communication component.

The other component, which will be referred to as the punishment com- ponent, consisted of an increase in face-to-face inspections in part of the municipalities that were selected to receive the communication component. Only registered establishments were visited by inspectors and inspections were targeted to enforce the formalization of workers. The implementation of the punishment component followed the launching of the communication component and inspection efforts took place within the semester of program implementation. As we shall show in section 5.2, only half of the municipalities actually experienced a rise in inspection rates and the intensity of inspections varied across the treated municipalities.

Apart from the 100,000 inhabitant threshold, the choice of treated municipalities by the two components was also based on the rate of employee informality, which was computed from the 2010 Census. However, despite the use of objective criteria, other dimensions such as local inspection capacity and political factors are also likely to have played a role in treatment choice. These dimensions are likely to be correlated with formalization outcomes and in section 4 we discuss our identification strategies.

3 DATA

Our analysis draws from four data sources. The 2010 demographic Census is used to compute population at the municipality level and the rate of informality of salaried workers across municipalities. The second data source is a data set that contains the number of labor inspections across the municipalities in Brazil. We had access to this data set from the labor inspection authority (SIT) and information was made available to us on a quarterly basis from the first quarter of 2012 (2012.q1) to the first quarter of 2016 (2016.q1).[11]

10. An english version of the letter translated by the authors is available in appendix A.

11. It is worth mentioning that in Brazil firms have to pay fines for non compliance with the various items of the labor code. In the case of non compliance with proper registering of hires and dismissals of workers, fines are stipulated per detected case.

The third source of data is an administrative data set named General Register of Employed and Unemployed (Cadastro Geral de Empregados e Desempregados – Caged). All registered establishments in Brazil are legally obligated to inform the Caged on a monthly basis about every new hire or separation that took place in the previous month. There is an end day within the month to declare this information to Caged and, after that day, establishments have to pay a fine for every hire or separation that was not declared. Although there can be other reasons for not declaring hires and separations before the due date (for instance, for having missed the date), one of them is that employers are actually informally hiring and discharging their workers. Put differently, it is likely that part of the hires and separations informed after the due date is triggered by inspections or because employers are (threatened to be) sued by workers, whose access to social security benefits (e.g., unemployment insurance) depends on demonstrating that they were previously formally employed. In fact, the inspection authority uses the information on hires and dismissals after the due date as an indicator to measure the performance of their inspection effort to formalize the labor force in various parts of the country. We use the total number of hires and dismissals after the due date – which we call regularizing hiring and regularizing dismissal – to construct some of our outcomes of interest (see below). To gauge potential impacts of the intervention on regular, for- mal labor demand, we also use information on the total number of hires and dismissals that are reported to Caged before the due date.[12] Data from Caged are aggregated at the municipality level on a quarterly basis from 2012.q1 to 2016.q1.

The fourth data source is also an administrative data set named Annual Roll of Social Information (Relação Anual de Informações Sociais – RAIS), it contains data on every single labor contract that all registered establishments had in the previous year. Virtually a census of the formal labor market in Brazil, RAIS data include basic information on the characteristics of workers – such as sex, age and education – as well as the industry and the municipality of establishments. We use the total number of workers at the municipality level from RAIS 2013 (i.e., one year before the start of the program) to construct our outcome variables. The total number of establishments at the municipality level from RAIS 2013 is used to compute the treatment variable for inspections. RAIS is also used to construct a set of covariates that enter some regressions in our empirical analysis.

12. As elsewhere, separations in Brazil occur due to quit, retirement or death of the worker, closure of the establishment, and dismissal of the worker by the employer. We only use information on this last type of separation to construct our outcome variables on regularizing and regular dismissals.

Based on these four data sources, we compute the following variables, all of which calculated at the municipality level for all municipalities in Brazil, as follows.

1) Outcome variables: Rate of regularizing hiring: ratio of quarterly number of hires that were declared after the due date (Caged) to the total number of workers in 2013 (RAIS); Rate of regularizing dismissals: ratio of quarterly number of dismissals that were declared after the due date (Caged) to the total number of workers in 2013 (RAIS); Rate of hiring: ratio of quarterly number of hires that were declared before the due date (Caged) to the total number of workers in 2013 (RAIS); Rate of dismissals: ratio of quarterly number of dismissals that were declared before the due date (Caged) to the total number of workers in 2013 (RAIS).

2) Treatment variables: Inspection rate: ratio of quarterly number of inspections to detect informal labor contracts (SIT) to the total number of establishments in 2013 (RAIS); List of treated municipalities (SIT).

3) Covariates: industrial composition of establishments at the one digit level, proportion of youths, and proportion of workers with at least high school – all computed on a yearly basis from RAIS 2012 to 2016. We also calculate the rate of informality of salaried workers from the 2010 Census.

4 EMPIRICAL STRATEGIES

Our goal is to measure the effects of a large-scale enforcement intervention on the decision of employers to formalize their workforce. As described in section 2, the intervention was targeted to tax-registered establishments located in a set of small and medium size municipalities with high informality rates. As the choice of this set of municipalities was not based on random assignment, there are potentially confounding factors that can bias the identification of the effects of the intervention on formal employment. Examples of such fac- tors are political influences that may have interfered in the choice of the treated municipalities and (unobserved) local differences across municipalities that affect both treatment choice (including the intensity of inspections) and formal employment. Operational capacity and quality of inspection offices throughout the country are examples of such local differences.

As aforementioned, the intervention contained two main components: the communication component, which was based on letters/e-mails sent by the government to employers in the treated municipalities, and a punishment component, which was carried out through increases in face-to-face inspections in a subset of municipalities. We are interested in investigating the effects of these two components. More specifically, our interest relies in assessing the effects of one component keeping the other constant. Thus, in order to deal with the potential influence of confounding factors and assessing the effects of each component of the program, we employ two different identification strategies.

One RDD, where we exploit the intervention's population cutoff to identify the program's effects in a quasi-experimental fashion. In principle, both the communication and the punishment components could vary at the program's cutoff point. However, as we shall show in section 5.1, our results evince that there was no change in inspection rates at the cutoff, so our impact estimates only unveil the effects associated with the communication component (for a given level of inspections).

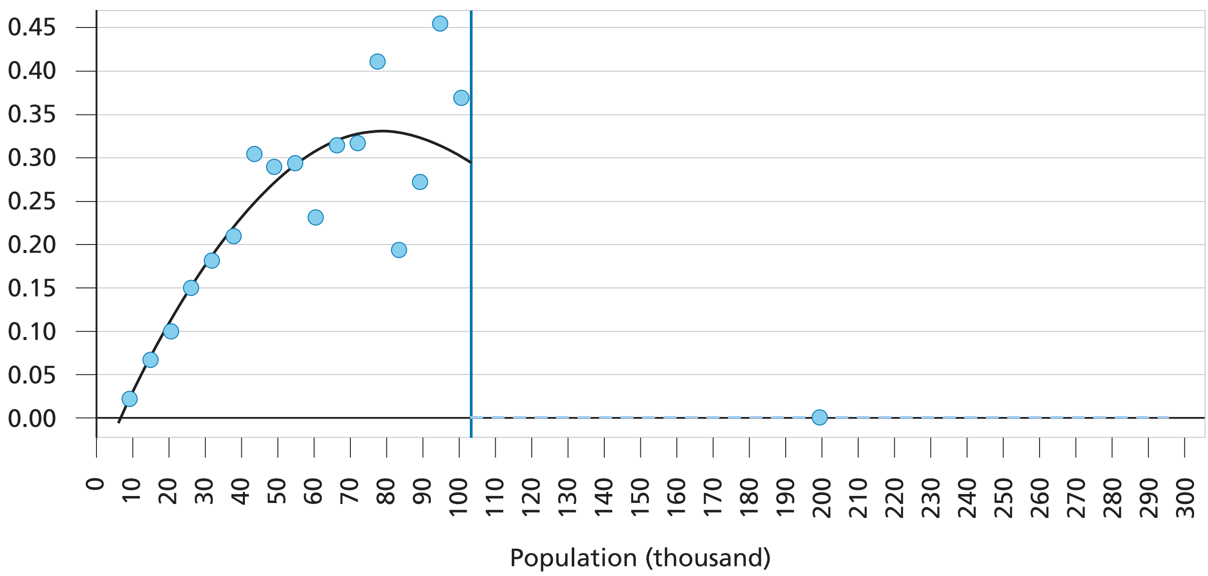
The second method we employ is DiD where we compare treated municipalities by the communication component and include municipality fixed effects to control for unobserved differences across the municipalities. Half of the municipalities experienced an increase in inspections while the other half did not. This allows us to measure the impacts of the punishment component (for municipalities whose employers were exposed to the communication component). The DiD model is also implemented for different levels of increase in inspections rates so that we can assess whether increments in the punishment components display heterogeneous effects. In the next two subsections we present the implementation details of these two methods.

4.1 Regression discontinuity

As previously described, a set of 527 municipalities with less than 100,000 inhabitants were selected to receive the program. Figure 2 shows the proportion of treated municipalities according to population size within bins of 5,000 inhabitants. As it can be seen, the 100,000 threshold was respected and the probability of being treated drops from around 35% to zero as the threshold is crossed from the left. This pattern has implications for the type of RDD that can be implemented. In fact, as the setting is neither purely sharp nor purely fuzzy, we are in a case that has been called partially-fuzzy design (Batistin and Rettore, 2008), also known as one-sided compliance.

In such setting, there are no always-takers and hence the typical identification of the effect only for compliers in the fully fuzzy RDD setting is capable to identify the effect for treated units. In other words, the average treatment on the treated (ATT) parameter can be identified through the same strategy employed to identify the local average treatment effect (LATE) parameter in the fully fuzzy context. Importantly, as Batistin and Rettore (2008) show, such ATT identification in partially fuzzy settings is achieved under the same mild conditions required in the sharp setting, i.e. that unobserved determinants of mean outcomes vary smoothly as a function of municipality population, particularly at the program's cutoff.

FIGURE 2
Proportion of treated municipalities within population-size bins (5,000)

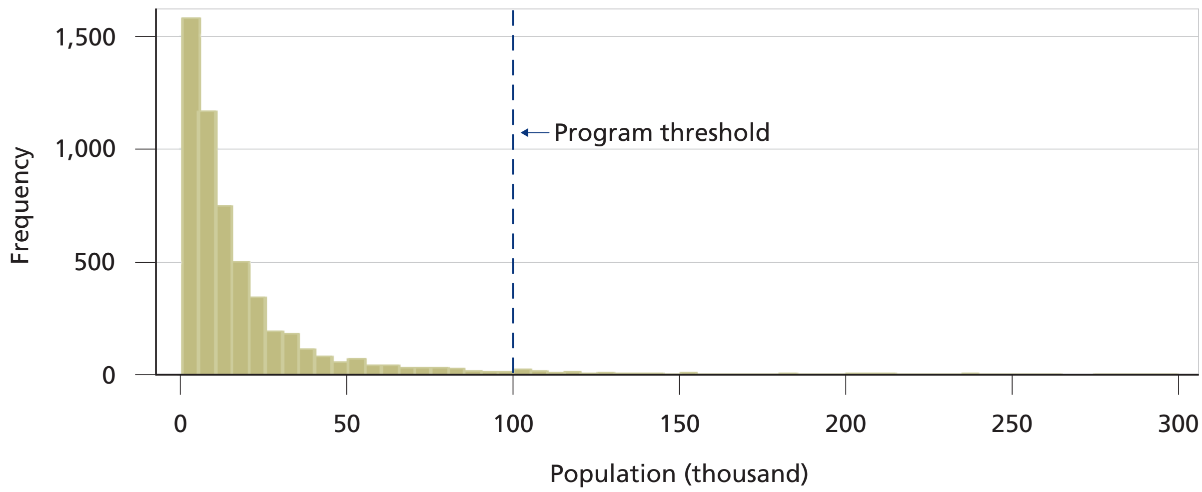


Source: SIT; Census 2010.
Authors' elaboration.
Obs.: Ratio of treated municipalities to the total number of municipalities within population-size bins of 5,000 inhabitants.

Although the manipulation of population size of municipalities is not plausible in our setting – it was based on the demographic Census –, one may ask whether the distribution of population across municipalities has any special feature around the 100,000 threshold. As can be seen in figure 3, there is no bunching of municipalities on either side of the threshold. In our partially-fuzzy setting, we implement the regression discontinuity design based on local polynomial regressions. We use the bias-corrected, robust estimator (RBC) proposed by Calonico, Cattaneo and Titiunik (2014). To further control for potential time-invariant unobserved factors at the municipality level, the model is estimated in first difference, which was computed as the difference between the quarters of program implementation (2014.q4/2015.q1) and the correspondent quarters of the previous years (2013.q4/2014.q1). We use different variance estimators – specifically the heteroskedasticity-robust plug-in (HC0) and the nearest neighbor (NN) estimators –, the local regression is specified both as linear and quadratic functions, and the triangular kernel is used in all regressions. Models are also estimated with and without the set of covariates specified in section 3. Table 1 shows that there was no difference in covariates at the program's cutoff, a result that indicates that municipalities were similar in observable characteristics at the point of identification.

FIGURE 3

Number of municipalities across population-size groups (bins of 5,000)



Source: 2010 demographic Census.

TABLE 1

Difference in covariates at the program's cutoff

Variables	OLP (1) Var.(HC0)	OLP (1) Var.(NN)	OLP (2) Var.(HC0)	OLP (2) Var.(NN)
Informality rate (2010)	0.0978	0.0962	0.0448	0.0447
	(0.0699)	(0.0585)	(0.0787)	(0.0651)
	[0.162]	[0.100]	[0.570]	[0.492]
Proportion at least high school	0.0390	0.0387	0.0414	0.0405
	(0.0519)	(0.0418)	(0.0587)	(0.0481)
	[0.452]	[0.355]	[0.481]	[0.400]
Proportion youth	-0.0265	-0.0253	-0.0148	-0.0165
	(0.0122)	(0.0103)	(0.0146)	(0.0138)

(Continues)

(Continuation)

Variables	OLP (1) Var.(HC0)	OLP (1) Var.(NN)	OLP (2) Var.(HC0)	OLP (2) Var.(NN)
	[0.0299]	[0.0145]	[0.313]	[0.231]
Proportion manufacturing	-0.0288	-0.0240	-0.0134	-0.0107
	(0.0225)	(0.0179)	(0.0293)	(0.0233)
	[0.200]	[0.179]	[0.648]	[0.647]
Proportion construction	-0.00384	0.000804	-0.00910	-0.00298
	(0.0122)	(0.00983)	(0.0130)	(0.0111)
	[0.753]	[0.935]	[0.483]	[0.788]
Proportion trade	0.0636	0.0575	0.0553	0.0597
	(0.0396)	(0.0315)	(0.0478)	(0.0402)
	[0.108]	[0.0678]	[0.247]	[0.137]
Proportion services	-0.0136	-0.00778	0.0163	0.0148
	(0.0303)	(0.0265)	(0.0384)	(0.0343)
	[0.654]	[0.769]	[0.670]	[0.666]
Proportion others	0.00187	-0.00486	-0.0388	-0.0325
	(0.0380)	(0.0294)	(0.0437)	(0.0311)
	[0.961]	[0.869]	[0.376]	[0.297]

Obs.: The informality rate corresponds to the ratio of the number of informal employees to the total number of employed workers. Youths are workers between 18 and 24 years old. OLP refers to the order of local the polynomial in regressions. Var.(HC0) and Var.(NN) corresponds respectively to the heteroskedasticity-robust plug-in and the nearest neighbor variance estimators. All regressions are run at the municipality level for the year before the program (2013). Robust standard errors are in parenthesis and p-values in square brackets.

4.2 DiD

We use the DiD method to estimate the impacts of the punishment component and its different intensities. We restrict the sample to municipalities that receive both components of the intervention and vary the intensity of inspections across the municipalities. Figure 4 displays the histogram of the distribution of changes in inspection rates across the treated municipalities. It shows that that the municipalities that experienced a positive rise in inspection rates during the program were those above the median of that distribution. The figure also shows that the rise in inspections was heterogeneous across the municipalities. In order to gauge the impacts of the punishment component we fix the control group as the subset of municipalities below the median and define three treatment groups of municipalities. The first group consists of all municipalities above the median (i.e., those that experienced some increase in inspection rates) and it is intended to capture the overall effect of the punishment component. The second and the third groups are formed by the municipalities whose change in inspection rates were above the 75th and 90th percentiles (0.054 and 0.135, respectively). The average increase in inspection rates for municipalities above the median was 0.094, whereas for those above the 75th percentile (90th percentile) was 0.161 (0.274). The average level one year before the program was 0.01, implying that the increase in inspections rates during the program was quite substantial.

Let m = 1, ..., 527 index the treated municipalities, t index quarters, and ymt represents an outcome variable of interest. All outcome variables are computed in first difference (denoted by Δ), which corresponds to the difference between the current quarter and the corresponding quarter of the previous year. In order to gauge the effects for distinct inspection intensities, the DiD regressions are separately run for sub-samples of the treated municipalities as specified in the previous paragraph. Letting p denote the p-percentile of the distribution of changes in inspection rates during the semester of program implementation (2014.q4/2015.q1), the model specification reads as follows:

$$\Delta y_{mt} = \alpha^p + \sum_{s=1}^{6} \rho_s^p + \sum_{s=1}^{6} (T_m^p . \beta_s^p) + \theta_m^p + \lambda_\tau^p + \varepsilon_{mt}, \tag{1}$$

where *s* represents semesters (2013.q2-2013.q4,...,2015.q4-2016.q1), T_m^p is a treatment dummy that assumes value one for municipalities whose changes in inspection rates were above the p = {0.50, 0.75, 0.90} percentile and value zero for municipalities below *p* = 0.50, ρ_s^p and β_s^p are dummies for semester *s*, θ_m^p is a municipality fixed effect, λ_τ^p is a year dummy, and ε_{mt} is a zero-mean disturbance term.

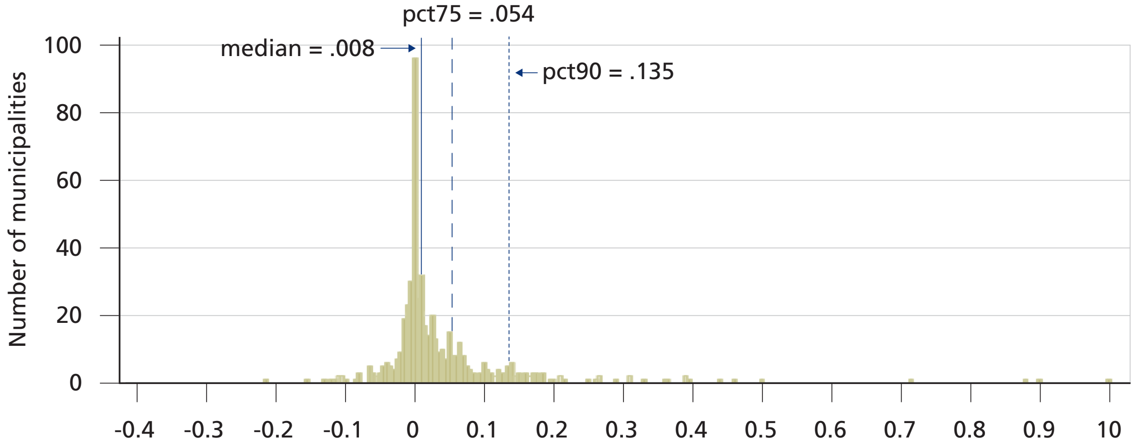
Note that, because equation (1) is run in first difference, the municipality fixed effect controls for differences in (linear) trends across the municipalities. This can be important as the treated municipalities were spread across a large and economically diverse country like Brazil and therefore subject to differential trends in their local economies.[13] We compute the F-test for βp for the semesters before the program to check for potential differences in outcomes between the treated groups and the control group. In all regressions, standard errors are clustered at the municipality level.

FIGURE 4

Number of treated municipalities across population-size groups (bins of 5,000)

13. Note too that first difference also absorbs seasonal factors that affect local labor markets.

according to the change in inspection rates



Source: SIT.

Obs.: Change in inspection rates was computed as the difference in rates between the quarters of program implementation (2014.q4/2015.q1) and the correspondent quarters of the previous years (2013.q4/2014.q1). The vertical lines mark the indicated percentiles of the distribution of the change in inspection rates.

5 RESULTS

This section reports the estimated effects of the program based on the two empirical strategies outlined in the previous section. Our main interest is to disentangle the effects of the communication and the punishment components of the intervention. In subsection 5.1 we present the results of the RDD method and show that the impact estimates capture the effects of the communication component. Subsection 5.2 presents the results of the DiD method where only the punishment component is varied. In order check the validity of each method, in each subsection we present results of placebo exercises that are based on data up to one year before the program's start.

5.1 Regression discontinuity and the communication component

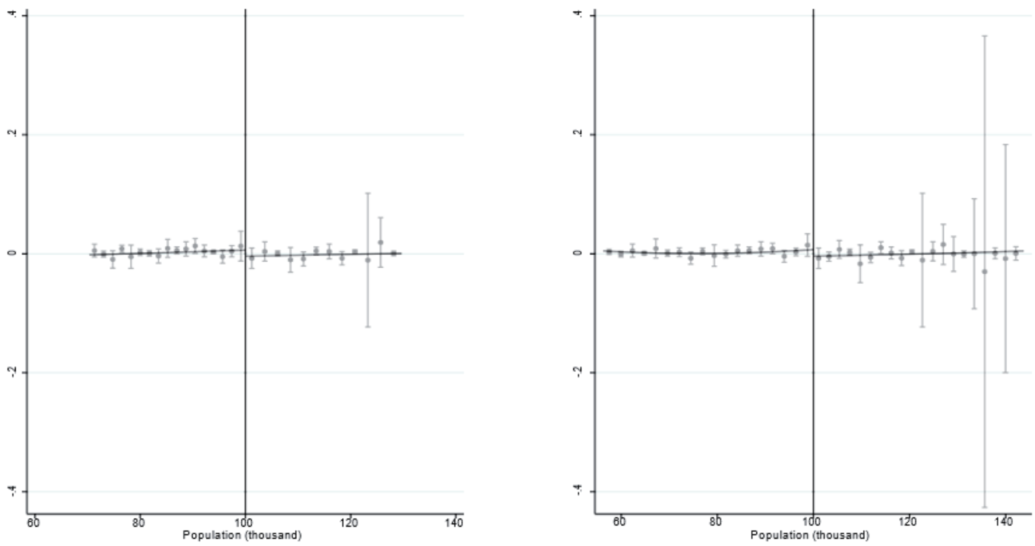
We start by exploiting the population discontinuity at the 100,000 threshold that defined program eligibility. As exposed in section 4.1, we employ the RDD method using different specifications to uncover program impacts. We first look at differences in inspection rates at the cutoff point. Figures 5 and 6 display respectively the results of local regressions for the level and the change in inspection rates when we use linear (left plot) and quadratic (right plot) polynomials in the specification.

As figure 5 shows the difference in inspection rates close to the cutoff point is near zero and not distinguishable from zero in statistical terms for both the linear and the quadratic specifications. The same result emerges in figure 6 for the change in the inspection rate.[14] Thus, based on these results, we conclude that there was no meaningful difference in the punish- ment component between the treated and the control municipalities near the population threshold for program eligibility. An important implication of this result is that any impact of the program on the outcome variables at the cutoff point should stem from the communication component alone. Put differently, since the inspection rates do not differ between the treatment and the control groups at the cutoff, any local effect of the intervention should derive solely from the mailing of the letter/e-mail to employers in treated municipalities.[15]

FIGURE 5

RDD plots for the level of inspection rate: linear and quadratic polynomials

5A – Polynomial order 1   5B – Polynomial order 2



Source: SIT; RAIS 2013.

Obs.: Plot for the level of inspection rate during the quarters of program implementation (2014.q4/2015.q1). The left (right) plot shows the results of a RDD local polynomial regression of the first (second) degree where the vertical line corresponds to the program's cutoff point. Robust, 95% confidence intervals are shown by the vertical segments in the plots.

14. In appendix B, we provide formal results os RDD regressions with various specifications. The estimates presented there confirm the results of figures 5 and 6.

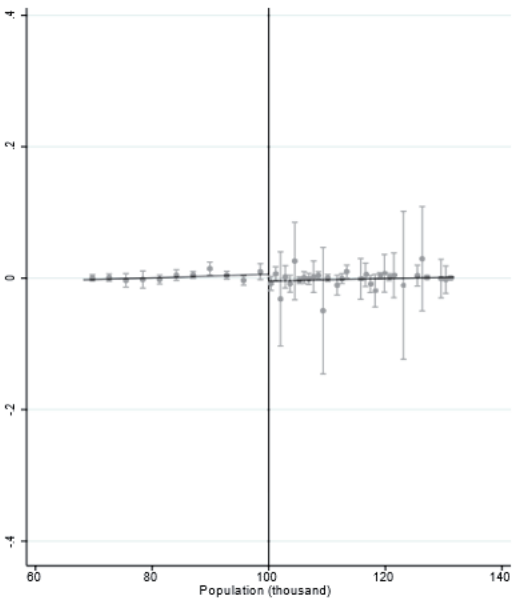
15. One should bear in mind that any local effect of the communication component is identified for a certain level of the inspection rate, that is, the level that prevailed close to the cutoff point (around 0.018 during the program).

We now move to present the estimated effects on the outcome variables of interest. Table 2 displays the impact estimates for the rate of regularizing hiring (top panel) and the rate of regularizing dismissals (bottom panel), while table 3 presents the results for the rate of hiring (top panel) and the rate of dismissals (bottom panel). Impact estimates when the RDD specification includes covariates are reported in appendix C and are very similar to the ones presented here.

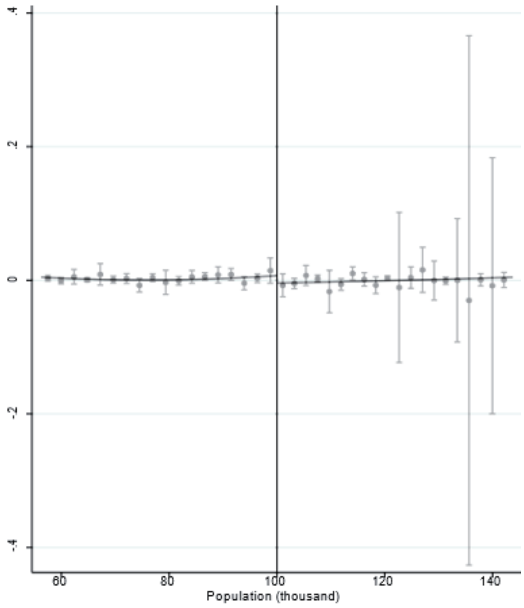
FIGURE 6

RDD plots for the change in the inspection rate: linear and quadratic polynomials

6A – Polynomial order 1



6B – Polynomial order 2



Source: SIT; RAIS 2013.

Obs.: Plot for the change in inspection rate between the quarters of program im- plementation (2014.q4/2015.q1) and the corresponding quarters of the previous years (2013.q4/2014.q1). The left (right) plot shows the results of a RDD local polynomial regression of the first (second) degree where the vertical line corresponds to the program's cutoff point. Robust, 95% confidence intervals are shown by the vertical segments in the plots.

The results presented in the top panel of table 2 show that all impact estimates for the rate of regularizing hiring are positive, statistically significant, and stable around 0.011 across all model specifications. This indicates that the intervention increased the rate of regularizing hires for the treated municipalities at the cutoff point. If we take the rate of regularizing hir- ing for the treated municipalities one year before the program (0.010), the magnitude of the effect implies that the intervention doubled the rate that employers regularize the hiring of their work force. The bottom panel of table 2 also shows positive and statistically meaningful estimates of program's impacts on the rate of regularizing dismissals. Estimates are stable around 0.010 across specifications and, as the pre-program level was 0.006, this im- plies that the rate at which the regularization of dismissals occurred almost doubled as a consequence of the intervention.[16]

Given we do not find evidence that employers in treated areas faced higher intensity of inspections near the cutoff, these results indicate that employers did react to the mailing of the letter/e-mail by the inspection authority. Thus, the communication component alone was strong enough to generate an increase in the formalization of previously hired and dismissed workers near the cutoff point of the intervention. As aforementioned, the missive contained elements of moral suasion and deterrence but did not explicitly threaten employers with future inspections. It is possible that the sole con- tent of the letter/e-mail was capable to change employers' behavior regarding their previous decisions to hire and discharge workers informally. If this explains the results, it may be that some employers were sensitive to moral suasion and others to deterrence factors (or both) but as the missive combined both elements, we cannot distinguish which element may have played a larger role. Another explanation is that the receipt of an official letter/e-mail from the government triggered a change in employers' perception about the probability of being caught and thus incited them to regularize the stock of informally hired and dismissed workers. It is difficult to say what mechanisms explain our results but the tax compliance literature is rich in showing that many factors influence agents' decisions to evade taxes when contacted by gorvernment authorities.[17]

16. In Brazil, employers have to pay firing costs when they formally dismiss workers with- out just cause. One may argue that the letter/e-mail sent by the inspection authority made employers regularize previous discharged workers and incur the firing costs. Alternatively, it could be that employers simply informed the government on the regularization of otherwise informal discharges and did not pay the firing costs. We do not have data to check whether firings costs raised in treated areas as compared to control areas.

17. The basic model is that of Allingham and Sandmo (1972) in which selfish firms optimally decide the amount of taxes to hide by equalizing the expected marginal costs (the perceived probability of being caught times the penalties) with the marginal benefits (the lower taxes). There is also a tax morale rationale based on individuals' intrinsic motivation to comply with the law due to deep moral judgment and/or willingness to conform with social norms and institutions. Another explanation is based on salience models (Chetty, Looney and Kroft, 2009) in which firms do not expect to be audited unless it is made salient to them, for instance through the receipt of information from tax authorities. Bergolo et al. (2023) propose an explanation based on a risk-as-feeling model in which taxpayers overreact to the threat of audits.

TABLE 2

RDD estimates for the change in the rate of regularizing hiring and dismissals

Variables	OLP (1) Var.(HC0)	OLP (1) Var.(NN)	OLP (2) Var.(HC0)	OLP (2) Var.(NN)
	Change in rate of regularizing hiring			
Robust	0.0106	0.0105	0.0109	0.0109
	(0.00555)	(0.00567)	(0.00554)	(0.00574)
	[0.0564]	[0.0650]	[0.0495]	[0.0576]
Observations Left	170	180	370	400
Observations Right	144	144	176	184
Bandwidth	21.5	22.2	36.9	38.3
	Change in rate of regularizing dismissals			
Robust	0.0106	0.0108	0.0099	0.0101
	(0.00508)	(0.00509)	(0.00517)	(0.00521)
	[0.0367]	[0.0345]	[0.0555]	[0.0539]
Observations left	246	258	432	452
Observations right	158	160	188	194
Bandwidth	27.5	28.3	40.4	41.3

Obs.: All estimates were computed using the bias-corrected robust estimator proposed by Calonico, Cattaneo and Titiunik (2014). The triangular kernel was used for all estimates. OLP is the order of the local polynomial. Var informs the type of variance: HC0 = heteroskedasticity-robust plug-in residuals; NN = heteroskedasticity-robust nearest-neighbor. Standard errors in parentheses and p-values in square brackets.

In contrast to the results for regularizing hiring and dismissals, none of the estimates in table 3 for the rates of hiring and dismissals are statistically significant at any conventional level. The absence of effects on these margins indicates that the communication component either did not affect regular, formal labor demand of treated employers or affected them in heterogeneous, compensating ways. Given the observed effects on the formalization previous hires and separations, one could expect that treated employers would adjust formal labor flows as well, for instance due to the costs incurred by the formalization of previous workers or to the deterrence elements of the communication component, including fears that inspections could follow.[18]

Our results refer to the period of program implementation and one may ask whether they change for longer horizons. Tables 4 and 5 report impact estimates respectively for the regularizing and regular outcomes one semester after the beginning of the program (i.e., 2015.q2/2015.q3). As it can be seen, estimates are not only close to zero but also statistically insignificant across all model specifications for all outcomes. Thus, employers only regularized the stock of informal workers in the aftermath of receiving the letter/e-mail, with regular, formal labor flows being insensitive to the communication com- ponent in the short and medium terms. One explanation for these results is that, as time passed, treated employers felt less threatened by the deterrence elements of the communication component – including the possibility of future inspections – and decided to move back to the "status-quo" behavior of employers in control areas. This change in behavior is compatible with what is known as "action and backsliding". It has been raised by Bosch, Fernandes and Villa (2015) to explain the vanishing of the effects of sending enforcement booklets to self-employed workers in Brazil. In the tax compliance literature, there are a set of studies that only find short-run or declining effects of official letters sent to individuals and firms (e.g., Pomeranz, 2015; DeBacker, 2018).

18. To offer a more formal explanation for our results we can consider a simple model in which firms live for two periods and face voluntary quits in the first period. In their first period, firms only hire workers from the unemployment pool to either formal (h_1^{uf}) or informal jobs (h_1^{ui}). In the second period, formal hiring comes from unemployment (h_2^{uf}) or from previously employed informal workers (h_1^{if}). In the absence of any intervention, in every period total formal hiring in our data is then given by $h_1^{uf} + h_2^{uf} + h_2^{if}$. If an enforcement intervention takes place, firms in their first period stop hiring informal workers ($h_1^{ui} = 0$) and increase formal hiring leading to $\alpha h_1^{uf} > h_1^{uf}$. When faced by the intervention, firms that are in their second period react moving informal workers to formal positions leading to $\delta h_2^{if} > h_2^{if}$. They also simultaneously decrease formal hiring from unemployment, so $\beta h_2^{uf} < h_2^{uf}$. Thus, total formal hiring in the intervention context is given by: $\alpha h_1^{uf} + \beta h_2^{uf} + \delta h_2^{if}$. Comparing the contexts with and without the intervention, we observe that hiring after the due date (h_1^{if}). In the absence of any intervention, in every period total formal hiring in our data is then given by $h_1^{uf} + h_2^{uf} + h_2^{if}$. If an enforcement intervention takes place, firms in their first period stop hiring informal workers ($h_1^{ui} = 0$) and increase formal hiring leading to $\alpha h_1^{uf} > h_1^{uf}$. When faced by the intervention, firms that are in their second period react moving informal workers to formal positions leading to $\delta h_2^{if} > h_2^{if}$. They also simultaneously decrease formal hiring from unemployment, so $\beta h_2^{uf} < h_2^{uf}$. They also simultaneously decrease formal hiring from unemployment, so $\beta h_2^{uf} < h_2^{uf}$. Thus, total formal hiring in the intervention context is given by: $\alpha h_1^{uf} + \beta h_2^{uf} + \delta h_2^{if}$. Comparing the contexts with and without the intervention, we observe that hiring after the due date increases ($\delta h_2^{if} > h_2^{if}$) and that it is possible that formal hiring before the due date does not change ($i.e., \alpha h_1^{uf} + \beta h_2^{uf} \lesseqgtr h_1^{uf} + h_2^{uf}$)

TABLE 3

RDD estimates for the change in the rate of hiring and dismissals

Variables	OLP (1) Var.(HC0)	OLP (1) Var.(NN)	OLP (2) Var.(HC0)	OLP (2) Var.(NN)
	Change in rate of hiring			
Robust	0.00992	0.00978	0.0135	0.0139
	(0.0127)	(0.0120)	(0.0125)	(0.0119)
	[0.434]	[0.417]	[0.280]	[0.243]
Observations left	308	324	354	420
Observations right	172	174	176	188
Bandwidth	32.3	34.0	35.9	39.6
	Change in rate of dismissals			
Robust	0.0126	0.0129	0.0135	0.0125
	(0.0129)	(0.0129)	(0.0118)	(0.0119)
	[0.329]	[0.318]	[0.249]	[0.296]
Observations left	354	378	304	330
Observations right	176	178	172	174
Bandwidth	36.2	37.2	31.7	34.3

Obs.: All estimates were computed using the bias-corrected robust estimator proposed by Calonico et al. (2014). The triangular kernel was used for all estimates. OLP is the order of the local polynomial. Var informs the type of variance: HC0 = heteroskedasticity-robust plug-in residuals; NN = heteroskedasticity-robust nearest-neighbor. Standard errors in parentheses and p-values in square brackets.

TABLE 4

RDD estimates for the change in the rate of regularizing hiring and dismissals one semester after the program

Variables	OLP (1) Var.(HC0)	OLP (1) Var.(NN)	OLP (2) Var.(HC0)	OLP (2) Var.(NN)
	Change in rate of regularizing hiring			
Robust	0.00461	0.00458	0.00411	0.00436
	(0.00431)	(0.00424)	(0.00438)	(0.00439)
	[0.285]	[0.281]	[0.348]	[0.321]
Observations left	432	462	496	532
Observations right	188	194	198	204
Bandwidth	40.2	41.8	43.8	46.0

(Continues)

(Continuation)

Variables	OLP (1) Var.(HC0)	OLP (1) Var.(NN)	OLP (2) Var.(HC0)	OLP (2) Var.(NN)
	Change in rate of regularizing dismissals			
Robust	0.00679	0.00678	0.00679	0.00689
	(0.00505)	(0.00507)	(0.00558)	(0.00568)
	[0.178]	[0.181]	[0.223]	[0.225]
Observations left	206	210	468	478
Observations right	146	146	194	196
Bandwidth	24.4	25.0	42.1	43.1

Obs.: All estimates were computed using the bias-corrected robust estimator proposed by Calonico et al. (2014). The triangular kernel was used for all estimates. OLP is the order of the local polynomial. Var informs the type of variance: HC0 = heteroskedasticity-robust plug-in residuals; NN = heteroskedasticity-robust nearest-neighbor. Standard errors in parentheses and p-values in square brackets.

As mentioned in section 4.1, our RDD identification strategy requires very mild conditions to be valid – specifically, that the conditional mean of the outcome variables behaves smoothly near the cutoff. Nonetheless, to provide higher credibility to our identification strategy, table 6 presents placebo exercises for all outcome variables in which we implement the same RDD regressions for the pair of quarters one year before the program start (i.e., 2013.q4/2014.q1). The absence of effects one year earlier provides evidence on the validity of our RDD strategy. As it can be seen from table, the estimates for both regularizing and regular outcomes are very small in magnitude and none are statistically meaningful across all model specifications.

TABLE 5

RDD estimates for the change in the rate of hiring and dismissals one semester after the program

Variables	OLP (1) Var.(HC0)	OLP (1) Var.(NN)	OLP (2) Var.(HC0)	OLP (2) Var.(NN)
	Change in rate of hiring			
Robust	0.0254	0.0268	0.0154	0.0153
	(0.0172)	(0.0160)	(0.0146)	(0.0139)
	[0.139]	[0.0944]	[0.293]	[0.269]
Observations left	330	384	302	354
Observations right	174	182	170	176
Bandwidth	34.5	37.7	31.6	36.3

(Continues)

(Continuation)

Variables	OLP (1) Var.(HC0)	OLP (1) Var.(NN)	OLP (2) Var.(HC0)	OLP (2) Var.(NN)
Change in rate of dismissals				
Robust	0.0180	0.0180	0.0187	0.0182
	(0.0133)	(0.0132)	(0.0131)	(0.0127)
	[0.176]	[0.172]	[0.153]	[0.153]
Observations left	322	330	308	352
Observations right	174	174	172	176
Bandwidth	33.8	34.3	32.8	35.9

Obs.: All estimates were computed using the bias-corrected robust estimator proposed by Calonico et al. (2014). The triangular kernel was used for all estimates. OLP is the order of the local polynomial. Var informs the type of variance: HC0 = heteroskedasticity-robust plug-in residuals; NN = heteroskedasticity-robust nearest-neighbor. Standard errors in parentheses and p-values in square brackets.

TABLE 6

RDD estimates for the change in the rate of regularizing hiring and dismissals and the rate of hiring and dismissals one year before program

Variables	OLP (1) Var.(HC0)	OLP (1) Var.(NN)	OLP (2) Var.(HC0)	OLP (2) Var.(NN)
Change in rate of regularizing hiring				
Robust	-0.00166	-0.00169	-0.00510	-0.00498
	(0.00589)	(0.00574)	(0.00703)	(0.00689)
	[0.778]	[0.769]	[0.468]	[0.469]
Observations left	218	226	226	228
Observations right	146	148	148	148
Bandwidth	25.4	25.9	25.9	26.0
Change in rate of regularizing dismissals				
Robust	-0.000424	0.000667	-6.09e-05	0.000493
	(0.00417)	(0.00505)	(0.00379)	(0.00400)
	[0.919]	[0.895]	[0.987]	[0.902]
Observations left	142	150	222	230
Observations right	132	138	148	150
Bandwidth	19.3	20.7	25.6	26.3

(Continues)

(Continuation)

Variables	OLP (1) Var.(HC0)	OLP (1) Var.(NN)	OLP (2) Var.(HC0)	OLP (2) Var.(NN)

Change in rate of hiring				
Robust	0.0181	0.0189	0.0139	0.0135
	(0.0118)	(0.0108)	(0.0120)	(0.0114)
	[0.124]	[0.0804]	[0.245]	[0.238]
Observations left	286	310	308	382
Observations right	162	174	172	178
Bandwidth	30.3	33.0	32.9	37.3
Change in rate of dismissals				
Robust	-0.00980	-0.00992	-0.0127	-0.0130
	(0.0103)	(0.00864)	(0.00947)	(0.00854)
	[0.340]	[0.251]	[0.180]	[0.128]
Observations left	136	148	322	354
Observations right	126	134	174	176
Bandwidth	18.4	19.7	33.7	35.9

Obs.: All estimates were computed using the bias-corrected robust estimator proposed by Calonico et al. (2014). The triangular kernel was used for all estimates. OLP is the order of the local polynomial. Var informs the type of variance: HC0 = heteroskedasticity-robust plug-in residuals; NN = heteroskedasticity-robust nearest-neighbor. Standard errors in parentheses and p-values in square brackets.

5.2 DiD and the punishment component

We now move to the DiD results which intend to measure the effects of increasing the inspection rate across municipalities. The sample is formed by all treated municipalities, so the impact estimates gauge the average effect of rising the intensity of the punishment component for employers that also received the e-mail/letter from the inspection authority.

Table 7 presents the results for the change in the rate of regularizing hiring (panel A) and the rate of regularizing dismissals (panel B) for the semester of program implementation and the two following semesters. Looking at the results for the rate of regularizing hiring during the program, the point estimate for the group above the median is positive (0.0025) but not statistically different from zero. However, for the groups above the 75th and the 90th percentiles estimates are positive (0.0041 and 0.0087) and statistically meaningful. The average pre-program levels (one year before the program) for the three groups was around 0.0075, so the effect sizes were substantial. The point estimates are monotonically increasing across the groups but one cannot reject they are pairwise equal on statistical grounds. Panel B of table 7 shows that the impact estimates for the rate of regularizing dismissals during the program are positive

(0.0028, 0.0049, and 0.0042) and statistically significant for all three groups we consider. Pre-program average levels were in the range (0.0033,0.0051), so the relative impacts of treatment were also substantial. Estimates are again not pairwise indistinguishable from each other in statistical terms. These results show that the employers did react to the punishment component of the intervention.[19] However, increasing the intensity of this component does not seem to have incrementally risen the formalization of previous hires and separations.

TABLE 7
DiD estimates for the change in the rate of regularizing hiring and dismissals

Variables	Above 50th percentile	Above 75th percentile	Above 90th percentile
Panel A: delta rate of regularizing hiring			
2014.4/2015.1	0.0025	0.0041	0.0087
	(0.0016)	(0.0021)	(0.0023)
	[0.1115]	[0.0455]	[0.0002]
2015.2/2015.3	-0.0008	-0.0016	0.0024
	(0.0016)	(0.0021)	(0.0020)
	[0.6038]	[0.4634]	[0.2352]
2015.4/2016.1	-0.0009	-0.0029	-0.0047*
	(0.0016)	(0.0021)	(0.0025)
	[0.5906]	[0.1681]	[0.0559]
R-squared	0.0058	0.0089	0.0137
F-test (pre-program)	1.296	2.021	0.315
P-value F-test (pre-program)	0.275	0.134	0.730
Panel B: delta rate of regularizing dismissals			
2014.4/2015.1	0.0028	0.0049	0.0042
	(0.0012)	(0.0018)	(0.0015)
	[0.0202]	[0.0070]	[0.0040]
2015.2/2015.3	0.0018	0.0019	0.0020
	(0.0012)	(0.0017)	(0.0017)
	[0.1314]	[0.2507]	[0.2435]

(Continues)

(Continuation)

Variables	Above 50th percentile	Above 75th percentile	Above 90th percentile
Panel B: delta rate of regularizing dismissals			

19. Although the literature in labor economics does not evaluate the effects of inspections including the communication component, most papers find positive (negative) impacts of inspections on formality (informality) (e.g., Almeida and Carneiro, 2009; 2012; Almeida, Carneiro and Narita (2015); Pignatti, 2018; Abras et al., 2018; Ulyssea, 2018; La Parra and Bujanda, 2020; Ponczek and Ulyssea, 2021).

	-0.0004	-0.0013	-0.0003
2015.4/2016.1	(0.0011)	(0.0016)	(0.0013)
	[0.7243]	[0.4321]	[0.8195]
R-squared	0.0083	0.0121	0.0126
F-test (pre-program)	0.0579	1.137	0.318
P-value F-test (pre-program)	0.944	0.322	0.727
Observations	6,312	4,740	3,792

Obs.: Impact estimates refer to municipalities that were above the indicated percentiles of the distribution of changes in inspection rates. The F-test and its correspondent P-value correspond to the joint test of coefficients for the two semesters before the program. Standard errors in parentheses and p-values in square brackets.

Results for the following semesters after program implementation show that program's effects were statistically zero for both the rate of regularizing hiring and the rate of regularizing dismissals. This suggests that employers responded to the increase in face-to-face inspections in the same fashion as in the case of treatment by the communication component alone, i.e. they formalize previous informal workers only in the aftermath of the intervention.

Table 8 presents impact estimates for the rates of hiring (panel A) and dismissals (panel B) for the semester of program implementation and the following two semesters. Overall, except for some estimates that are weakly statistically significant, all other estimates for both outcomes and periods are not significant at conventional levels. In addition, the average levels for the two outcome variable one year before the intervention were respectively in the range (0.0508,0.0750) and (0.0462,0.0774), so impact estimates represent less than 10% of pre-program levels.

The results of this section show that even when a deterrence factor such as face-to-face inspections was in place, the reaction of treated employers was to formalize the stock of informal workers without changing their regular, formal labour demand. One could expect that employers would adjust formal labor flows due to the costs of the formalization of previous workers or to the deterrence force of the punishment component. However, the evidence suggests that either the punishment component was not strong enough to change employers' decisions regarding new formal hires and separations or that there were heterogeneous, compensating effects on these margins across types of firms.[20]

20. The simple model presented in footnote 21 rationalizes this second explanation.

As in section 5.1, the effects are also only observed for the short run. Thus, it seems that increases in the punishment component were not capable to change the perceived probability of employers of being caught in the longer run. One explanation is that employers (over)react to the salience of enforcement events when they occur and then backslide to their usual compliance behavior.

The bottom of panels A and B of tables 7 and 8 show that the F-tests for the coefficients of pre-program periods are not significant for all outcome variables and groups of treated municipalities. This indicates that there were no differential trends before the program between each treatment group and the control group. Table 9 presents results of a placebo exercise in which a DiD regression for each outcome variable is run for the period up to one year before the beginning of the intervention. The results show that estimates are very small and not statistically significant for all outcome variables. The evidence thus indicates that the DiD method was a valid identification strategy.

TABLE 8

DiD estimates for the change in the rate of hiring and dismissals

Variables	Above 50th percentile	Above 75th percentile	Above 90th percentile
	Panel A: delta rate of hiring		
2014.4/2015.1	0.0074	0.0096	0.0036
	(0.0041)	(0.0054)	(0.0088)
	[0.0747]	[0.0752]	[0.6844]
2015.2/2015.3	0.0077	0.0152	0.0062
	(0.0053)	(0.0079)	(0.0080)
	[0.1436]	[0.0551]	[0.4369]
2015.4/2016.1	0.0002	0.0088	0.0057
	(0.0042)	(0.0054)	(0.0070)
	[0.9702]	[0.1015]	[0.4195]
R-squared	0.0185	0.0145	0.0196
F-test (pre-program)	1.528	1.317	0.363
P-value F-test (pre-program)	0.218	0.269	0.696

(Continues)

(Continuation)

Variables	Above 50th percentile	Above 75th percentile	Above 90th percentile
	Panel B: delta rate of dismissals		

	0.0033	-0.0004	0.0010
2014.4/2015.1	(0.0039)	(0.0057)	(0.0053)
	[0.3986]	[0.9457]	[0.8458]
	0.0004	-0.0020	-0.0007
2015.2/2015.3	(0.0045)	(0.0067)	(0.0080)
	[0.9374]	[0.7654]	[0.9275]
	-0.0034	-0.0010	0.0084
2015.4/2016.1	(0.0043)	(0.0060)	(0.0046)
	[0.4257]	[0.8654]	[0.0708]
R-squared	0.0158	0.0118	0.0180
F-test (pre-program)	0.180	0.330	1.883
P-value F-test (pre-program)	0.835	0.719	0.154
Observations	6,312	4,740	3,792

Obs.: Impact estimates refer to municipalities that were above the indicated percentiles of the distribution of changes in inspection rates. The F-test and its correspondent P-value correspond to the joint test of coefficients for the two semesters before the program. Standard errors in parentheses and p-values in square brackets.

6 CONCLUSION

In this paper, we assess the effects of a large-scale enforcement intervention on formal hiring and dismissals in Brazil. The initiative was implemented in a set of municipalities with less than 100,000 inhabitants and was conducted by the inspection authority in the country. The intervention comprised two components. One was sending a letter or e-mail to tax-registered employers whose content contained elements of moral suasion (e.g., the social benefits of formal employment) and deterrence (e.g. cross checking of information with other government bodies). We call this component the communication component. The second component was an increase in face-to-face inspections that took place in a subset of the municipalities that were exposed to the first component. We name this component the punishment component. The main objective of the paper is to isolate the effects of varying one component keeping the other constant. To the best of our knowledge, this is the first study that isolates these two effects from an enforcement intervention that reached a large number of registered employers irrespective of size in a country.

TABLE 9

DiD estimates for the change in the rate of regularizing hiring and dismissals and the rate of hiring and dismissals one year before program

Variables	50th percentile	75th percentile	90th percentile
	Change in rate of regularizing hiring		
Coefficient	-0.0013	-0.0030	-0.0004
	(0.0016)	(0.0022)	(0.0026)
	[0.4157]	[0.1779]	[0.8885]
Observations	2,630	1,975	1,580
R-squared	0.1514	0.1477	0.1373
	Change in rate of regularizing dismissals		
Coefficient	-0.0003	-0.0011	-0.0013
	(0.0013)	(0.0017)	(0.0016)
	[0.8126]	[0.5281]	[0.4118]
Observations	2,630	1,975	1,580
R-squared	0.1706	0.1745	0.1764
	Change in rate of hiring		
Coefficient	-0.0053	-0.0067	-0.0069
	(0.0047)	(0.0071)	(0.0118)
	[0.2631]	[0.3444]	[0.5605]
Observations	2,630	1,975	1,580
R-squared	0.2325	0.2225	0.2270
	Change in rate of dismissals		
Coefficient	0.0008	-0.0016	0.0077
	(0.0050)	(0.0079)	(0.0045)
	[0.8707]	[0.8355]	[0.0881]
Observations	2,630	1,975	1,580
R-squared	0.3038	0.3048	0.3112

Obs.: Estimates refer to municipalities that were above the indicated percentiles of the distribution of changes in inspection rates between the period of the program and one year earlier. Standard errors in parentheses and p-values in square brackets.

It is likely that the choice of the treated municipalities was influenced by unobserved (to the analyst) dimensions such as political factors and the local capacity of the inspection authority. Thus, to circumvent potential endogeneity problems, we employ two distinct identification strategies. The first is RDD which exploits the 100,000 population cutoff of the program. We find that there was no variation in inspection rates at the cutoff, which allows us to isolate the effects of the communication component. The second strategy is DiD which is applied to the treated municipalities by the communication component but with different intensities of the punishment component.

We use data from various sources, all aggregated at the municipality level. Inspection rates are constructed from data on the total number of inspections specifically targeted to check the formalization of workers in the country. Outcomes variables come from administrative data on two types of hires and dismissals. The first is the number of hires and dismissals that are reported to the government after the due date to declare them. Following the inspection authority, we interpret them as indicators of changes in employers' decisions to formalize their previously employed informal workers. The second type is the number of new, regular hires and dismissals, i.e. those informed before the due date. Inspection rates as well as flow rates of hiring and dismissals were computed using data from another data source which contains information on virtually all formal establishments in the country.

The results from the RRD method show that the communication component substantially increased the formalization of hires and dismissals of previous informal employees but no effects were detected on regular, formal labor flows. The DiD results also evince positive effects of the punishment component on previous informally hired and dismissed workers but no impacts on regular, formal labor flows. Somewhat unexpected, the effects of different intensities of the punishment component were not distinct from each other on statistical grounds. Both RDD and DiD results show that none of the two components was capable to produce long term effects. It seems thus that employers did react to the communication and the punishment components by formalizing the stock of previous informal workers in the short run but did not adjust their regular, formal labor demand either in short or the long term. One possible explanation is that that each component (keeping the other constant) only made salient the enforcement of the law when employers were exposed to them and, after that, they moved back to their usual compliance behavior.

Given that the two components have very different costs of implementation, it would be interesting from a policy perspective to implement a randomized control trial with different arms, where one would be based on sending letters/e-mails to employers (possibly differentiating moral suasion and deterrence elements), another based on actual inspections, and a third containing both components. This is left for future research.

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APPENDIX A

Mr./Mrs. Entrepreneur,

The Ministry of Labor and Social Security will intensify the National Plan to Combat Informality of Employees (Plano Nacional de Combate à Informalidade dos Trabalhadores Empregados – Plancite), aimed at increasing the number of employees with formal contracts, to provide their access to labor rights and social security benefits and contribute to the promotion of greater social justice.

The formalization of the employee must occur immediately upon admission, upon signature of the Work and Social Security Card (Carteira de Trabalho e Previdência Social – CTPS), from the book/card record of registration, materializing with the provision of the declaration to the General Registry of Employees and Unemployed (Cadastro Geral de Empregados e Desempregados – Caged) as per specific instructions established by this ministry.

Informality leads to losses not only to the worker, but to the whole Brazilian society. Those who keep their employees in informality also harm your competitors and compromises the future of our country.

Several mechanisms have already been created that reduced the tax bur- den and simplified the hiring of employees, such as payroll exemption for various economic activities, the Simples Nacional (recently expanded), in addition to simplifying opening or formalization of the business.

The ministry is improving its legal role. With Plancite, it intensified the use of fiscal intelligence, increased the crossing of important information with others public bodies, and modernized the monitoring of informality throughout Brazil, providing elements for a much better specific fiscal action by municipality, by economic activity, and by company.

This expanded the coverage of labor inspection and optimized their actions, further increasing their presence throughout the national territory, especially in outskirts of large urban areas, in the municipalities of the interior and in rural areas.

In addition to a fine, the employer is subject to losing the benefit of the Simples Nacional when omitting the worker from the payroll or others labor documents.

Everyone needs to contribute towards the end of informality, aiming a more just society for employers and workers.

Yours sincerely,
Ministry of Labor and Social Security.

APPENDIX B

TABLE B.1
RDD estimates for the level and change in inspection rates

Level of Inspection Rate	OLP (1) Var.(HC0)	OLP (1) Var.(NN)	OLP (2) Var.(HC0)	OLP (2) Var.(NN)
	0.0165	0.0165	0.0148	0.0151
Robust	(0.0122)	(0.0111)	(0.0124)	(0.0115)
	[0.175]	[0.136]	[0.233]	[0.190]
Covariates	N	N	N	N
Observations left	272	272	496	546
Observations right	162	162	198	204
Bandwidth	29.7	29.7	43.8	46.5
	0.0134	0.0103	0.0198	0.0163
Robust	(0.0159)	(0.0130)	(0.0172)	(0.0147)
	[0.399]	[0.430]	[0.250]	[0.268]
Covariates	Y	Y	Y	Y
Observations left	92	112	150	178
Observations right	108	110	138	144
Bandwidth	14.9	16.5	20.4	22.0
Change in inspection rate	**OLP (1) Var.(HC0)**	**OLP (1) Var.(NN)**	**OLP (2) Var.(HC0)**	**OLP (2) Var.(NN)**
	0.0260	0.0245	0.0253	0.0249
Robust	(0.0150)	(0.0139)	(0.0161)	(0.0154)
	[0.0822]	[0.0764]	[0.118]	[0.108]
Covariates	N	N	N	N
Observations left	306	276	700	650
Observations right	172	162	214	210
Bandwidth	31.8	30.0	52.2	50.4
	0.0258	0.0250	0.0230	0.0211
Robust	(0.0170)	(0.0158)	(0.0183)	(0.0173)
	[0.129]	[0.113]	[0.210]	[0.224]
Covariates	Y	Y	Y	Y
Observations left	58	58	138	148
Observations right	88	88	128	136
Bandwidth	10.5	11.0	18.9	20.3

Source: ????

Obs.: 1. RDD – regression discontinuity design.

2. All estimates were computed using the bias-corrected robust estimator proposed by Calonico, Cattaneo and Titiunik (2014). The triangular kernel was used for all estimates. OLP is the order of the local polynomial. Var informs the type of variance: HC0 = heteroskedasticity-robust plug-in residuals; NN = heteroskedasticity-robust nearest-neighbor. The covariates are the industrial composition of establishments at the one-digit level, the proportion of youths, and the proportion of workers with at least high school. Standard errors in parentheses and p-values in square brackets.

APPENDIX C

TABLE C.1

RDD estimates for the change in the rate of regularizing hiring and dismissals with covariates

Variables	(1) OLP (1) Var.(HC0)	(2) OLP (1) Var.(NN)	(3) OLP (2) Var.(HC0)	(4) OLP (2) Var.(NN)
Change in rate of regularizing hiring				
Robust	0.0125	0.0128	0.0122	0.0132
	(0.00661)	(0.00683)	(0.00704)	(0.00753)
	[0.0589]	[0.0607]	[0.0821]	[0.0800]
Covariates	Y	Y	Y	Y
Observations left	94	110	178	204
Observations right	108	110	144	146
Bandwidth	15.2	16.3	22.1	24.4
Change in rate of regularizing dismissals				
Robust	0.0094	0.0105	0.0082	0.0093
	(0.00543)	(0.00562)	(0.00518)	(0.00537)
	[0.0827]	[0.0623]	[0.115]	[0.0831]
Covariates	Y	Y	Y	Y
Observations left	90	112	190	216
Observations right	108	114	144	146
Bandwidth	14.8	16.7	22.8	25.2

Source: ????

Obs.: 1. RDD – regression discontinuity design.

2. All estimates were computed using the bias-corrected robust estimator proposed by Calonico, Cattaneo and Titiunik (2014). The triangular kernel was used for all estimates. OLP is the order of the local polynomial. Var informs the type of variance: HC0 = heteroskedasticity-robust plug-in residuals; NN = heteroskedasticity-robust nearest-neighbor. The covariates are the industrial composition of establishments at the one-digit level, the proportion of youths, and the proportion of workers with at least high school. Standard errors in parentheses and p-values in square brackets.

TABLE C.2

RDD estimates for the change in the rate of hiring and dismissals with covariates

Variables	OLP (1) Var.(HC0)	OLP (1) Var.(NN)	OLP (2) Var.(HC0)	OLP (2) Var.(NN)
		Change in rate of hiring		
Robust	0.0169	0.0164	0.0175	0.0180
	(0.0134)	(0.0123)	(0.0146)	(0.0127)
	[0.208]	[0.185]	[0.229]	[0.156]
Covariates	Y	Y	Y	Y
Observations left	106	142	178	266
Observations right	108	132	144	160
Bandwidth	16.0	19.3	21.9	28.9
		Change in rate of dismissals		
Robust	0.0184	0.0203	0.0136	0.0163
	(0.0129)	(0.0139)	(0.0121)	(0.0128)
	[0.152]	[0.143]	[0.258]	[0.200]
Covariates	Y	Y	Y	Y
Observations left	86	112	148	170
Observations right	108	114	136	144
Bandwidth	14.1	16.6	20.1	21.5

Source: ????

Obs.: All estimates were computed using the bias-corrected robust estimator proposed by Calonico, Cattaneo and Titiunik (2014). The triangular kernel was used for all estimates. OLP is the order of the local polynomial. Var informs the type of variance: HC0 = heteroskedasticity-robust plug-in residuals; NN = heteroskedasticity-robust nearest-neighbor. The covariates are the industrial composition of establishments at the one-digit level, the proportion of youths, and the proportion of workers with at least high school. Standard errors in parentheses and p-values in square brackets.

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